into two main divisions. In the present work, although the notation of the integral calculus has not been used, yet the method of integration has been explained, and, in fact, applied to the solutions of some problems, such as those of finding moments of inertia, centres of pressure, &c. The treatment of the subject-matter on the whole has, however, been developed by very simple mathematical methods, and although the book is published in the "Advanced Series," it should not for that reason be reckoned as of too high a standard for elementary readers. Indeed the author has inserted several useful introductory chapters to prepare such readers as those who have only an elementary knowledge of the mechanics of solids; thus we are treated to chapters on units, principles of statics, uniform, circular, and harmonic motions. In these chapters, and also in the others dealing with the mechanics of fluids, the author seems to be especially clear in his explanations, and his remarks are in many cases accompanied with diagrams and illustrations, which are always of great help to the reader studying the subject for the first time.

In a work of this kind a student can best obtain a good grasp of the subject by supplementing his study of the text with the working out of numerous typical examples. Those here inserted should be specially useful in this direction, and in many cases they have been divided into two series, the second being of a more difficult type than the first; many of the examples are taken from such sources as the examination papers set at South Kensington, and those for the Civil Service and the Universities.

As a text-book for science schools, and suitable for those wishing to get a thorough insight into the subject, the book will be sure to find favour.

*How to Manage the Dynamo.* By S. R. Bottone. (London: Whittaker and Co.)

IN this book the author gives, in thirty-five short pages, a few practical directions for the installation and management of a dynamo. The instructions given are intended for the use of engineers who have no knowledge of electrical work, but are called upon to undertake the management of a dynamo. It is hardly possible in such circumstances to frame directions which can be intelligently carried out. The dynamo attendant, like every other person in charge of machinery, must really learn by an experience which no manual can replace. The instructions in the text are, however, plainly and simply stated, and deal with some of the more important points connected with the care of dynamos.

An appendix is devoted to the explanation of technical terms used in electricity, and makes up about twelve of the total forty-seven pages of the book. It can hardly be regarded as satisfactory, either in point of accuracy or extent. It is more of a series of detached explanatory statements than a "table of definitions," as the author calls it in the preface.

## LETTERS TO THE EDITOR.

*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### The Cloudy Condensation of Steam.

(1) THE fault I have to find with Mr. Bidwell (NATURE, December 28, p. 212, 1893) and others who are aiming to revive the chemical hypothesis tentatively broached by the late R. von Helmholtz (*Wied. Ann.* xxxii. p. 1, 1887. *cf.* p. 7 *et seq.*), is that these gentlemen are abandoning a tried theory when its capabilities are far from exhausted. To begin at once with the most recent contribution to our knowledge of the subject, Mr. Bidwell's observation, let a series of excessively small condensation nuclei be enclosed in a vessel without further interference. They were active before being put there; but after remaining imprisoned for a sufficient length of time, Mr. Bidwell finds them shorn of their power to produce condensation in supersaturated steam. Now, I ask, is it at all probable that these particles will remain distinct throughout the whole time of confinement? I think not. Take the familiar case of fine clay suspended in water. What goes on during subsidence is an agglomeration of particles. This process may be enormously accelerated by small additions of acid or almost any other foreign matter to the water. In ether the speed of agglomeration is so marked, that the (dry) mud which may be suspended in water for weeks, or even months, falls out of ether like so many grains of sand. Witness, furthermore, the case of any chemical precipitate. Those little solid corpuscles were originally all but molecular in size; yet they grew with such enormous rapidity to so enormous a bulk (relatively speaking), that the formed precipitate subsides quickly (in most cases) even in water. Think of those monstrous clots of fresh chloride of silver; think, too, that they were being built up of individual molecules in an instant and before one's very eyes. Then why should condensation nuclei be so good as to remain distinct indefinitely?

In brief, it is not probable that the condensation nuclei[1] if once produced can subsist in their original degree of comminution; for whatever the nature of the acting forces may be, this finely divided matter is in a highly potentialised state relatively to coarser matter, and there will be dissipation of energy by mere mechanical agglomeration even when chemical action is excluded.

(2) Let us look for further confirmation at the size which these active nuclei must necessarily have, remembering that the condensation in question, when properly viewed, always appears coloured.[2] Such condensation presupposes droplets nearly all of the same size; of a size, moreover, which we can reasonably conjecture to lie somewhere between ·000,004 centimetre and ·000,040 centimetre, depending on the colour selected. This implies that the nuclei which induce coloured condensation must all be of the same size (certainly an improbable condition), or that they must be small even when compared with the small droplets stated. If it were not so, large drops would condense on large nuclei, and small drops on small nuclei (keeping in mind that the whole process of condensation is virtually instantaneous), and there could be no prevailing dimension, and consequently no intense colour.

But it will be asked, will these excessively small particles meet the Kelvin conditions of condensation? Assuredly. One may estimate that pure, dust-free, unconfined steam at 100° would require a pressure of the order of ten or more atmospheres to condense it. Add to this dust particles less than ·000,001 centimetre in diameter, and the pressure sinks to 15 centimetres of mercury; in case of particles ·000,010 centimetre in diameter, to one or two centimetres of mercury; in other words, to pressure increments[3] certainly met with in steam jets. The fact that nuclei of a few hundred molecular diameters are needed is the very feature of these experiments, and explains why smoke and other coarse material is useless, and why the condensation-producing dust must be so highly specialised.

(3) Nuclei, it is true, are often equally active if derived from liquids, the mixture obtained by passing air over very strong sulphuric acid being a notable example. Let us remember, however, that these active liquid bodies (acids all of them, while ammonia is not active) may form sulphate of ammonia in contact with atmospheric air. But if this view of the case be unsatisfactory, is there anything in Lord Kelvin's well-known equation connecting vapour tension and curvature which asserts that the nuclei *must* be solid?

(4) I have equally great difficulties in submitting to the diverse electrical hypotheses put forth, and I cannot understand why so astute and sound-minded an investigator as Mr. Aitken should hazard his birthright for that mess of electrical pottage. It is certain that air passing across an active spark gap will produce condensation; but it is equally certain that no electric field will

[1] It must not be forgotten that the imprisoned nuclei are mixed with the nuclei normally present in air. Whether the mixture is pronounced active or not, depends on the sensitiveness of the test applied. Thus there is room for an error of judgment. I recur to this in § 6.

[2] I here include the opaque field mentioned in Mr. Aitken's and in my own articles, since it obeys clean cut colour laws.

[3] Pressures between 1 and 80 centimetres above the atmosphere were necessary in my work.



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be wafted down a metallic tube three or more metres long by a draught of air—a crucial test which I believe to have been the first to make, though Mr. Bidwell does not affirm it. If charged particles, dissociated particles, and the like are sucked down there, I must insist that the said charged or dissociated particle—whatever befall it—is a particle still! If it is not small enough, it will fail to stimulate condensation in any case or theory. If it is small enough, it *must* produce nuclear condensation *per se*, no matter how the particle is otherwise conditioned.

Aside from this, the electrical theory becomes more seriously entangled by the fact that the jet itself is no mean generator of electricity. I made a few tests on this point in the line of Faraday's classical researches, and obtained marked charges from the jet, increasing with its intensity.

(5) I cannot claim much indulgence for my experiments in air filtration, though I went through them laboriously enough. But the work taught me, at least, the extreme elusiveness of such a thing as "dust-free" air. Naturally I am biassed, therefore, in regard to arguments, in the present case, based on filtration. Suppose an oxyhydrogen flame burning in filtered air is an active dust producer. The question at once arises on what kind of hearth is the flame kindled: if it burns from a glass tube, then sodium is probably volatilised; if from a metallic tube, then the metal is similarly in danger; if from no base at all, then where is the flame? In what does the remarkable activity of flames really consist? Most reasonably, it seems to me, in this, that particles therein entrapped are (as a rule) at once *volatilised*, so that for each single particle we have now a whole cloud of active nuclei precisely the kind wanted in § 2; *i.e.* myriads of them, all in that extreme degree of tenuity which best promotes condensation. In my work, glowing smokeless charcoal often did better service than flames. Alkalies are here ready for volatilisation. In general, hot flames are more active than colder flames (Helmholtz), and they should be where disgregation is needed.

Finally, a word with regard to red-hot platinum. "Here," says Mr. Bidwell, "there can be no nuclei formed of products of combustion, for there is no combustion, simply ignition and incandescence." Is it possible that Mr. Bidwell is not aware that red-hot platinum is particularly remarkable for scattering small solid particles from its surface? If so, he has narrowly escaped being overwhelmed by the literature of the subject. Aside from this, what may be the nature of platinum dissociated at red heat?

(6) To conclude: I cannot discern that the proof of anything beyond condensation of supersaturated steam, induced by mere "inert" nuclei, has yet been given. Nothing is even said of atmospheric air, which indoors or out, stagnant or fresh, is always active; nor is it even hinted at that the effect of dust is merely an *accentuation* of the effect produced by such air; that dust-stimulated condensation differs merely in degree, by no means in kind, from jet condensation in air. Air nominally purified needs only a higher degree of supersaturation to evoke condensation running through the whole gamut of colours. There are no hard drawn lines. I wish there were. Indeed, I wish the proof in question could or had been adduced, for, together with my colleague Prof. F. H. Bigelow, I am well aware of the important meteorological consequences to which this result would lead. But before I can break away from the time-honoured point of view, so safely trussed by the Kelvin formula, the experimental evidences forthcoming must be as rigorously "dust-foll" as the clear conscience with which I am disposed to admit them.

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## The Origin of Lake Basins.

THE question of the origin of lake basins has again been raised, and, unfortunately, there is even now the same diametrical opposition between the views of the glacialists and their adversaries. Though the old lines of argumentation, perhaps, have not been followed out with sufficient perseverance, new starting-points certainly seem desirable. I should therefore like to challenge the criticism in your columns on a demonstration of the glacial origin of the greatest fjord basins in Norway.

On the western coast of Norway we have the well-known series of great fjords, generally of enormous depth, about 400 fathoms in Hardangerfjord, 600 in Sognefjord, and 300 in Nordfjord, to take only the greatest. The heads of the innermost branches are nowhere at a greater distance than some twenty miles from the watershed of the country, and the necks between them nowhere as much as forty miles. When for some reason, change of climate or rise of land, the snow began to gather on the neighbouring plateau, the highest in Scandinavia, glaciers would creep down the steep slopes and valleys, and immediately get to the deep fjords. But here the glacier ends must needs be carried away or dissolved as fast as they came on. By the necessarily slow growth of the narrow névés, it is impossible that the glaciers could advance at once with so great dimensions that the fjords were not able to master them successively. Over the narrow necks between them an ice cap certainly might push farther out, but as there is less than twenty miles to draining outlets on either side, this cap could attain no considerable thickness, and bring no great ice flow westwards from the high land behind. On the lower foreland, near the coast-line, there is nowhere sufficient gathering basin for a great névé, and the small fjord branches could easily drain their eventual surplus. My opinion is, then, that no great inland ice could possibly advance farther west in Norway than to the close set row of fjord-heads.

It may be said that the whole country in the great Ice Age was so much elevated that the fjords were only dry valleys. But no amount of elevation could ever drain fjord troughs generally 300 to 600 fathoms deep, and only 100 to 150 wide at the brim. And an adequate differential lift of the inner side of the troughs would give the old palæozoic mountains in Norway the height of the youngest mountain ranges, and is on the face of it impossible. It may be further said that ice from the high land behind the watershed might have contributed to the supply, and that the glaciers then would be large enough. But even in this case they must needs commence as small ice tongues, which would be cut off successively; and the boulders show that no transport from any distance behind the watershed took place during the great Ice Age.

The idea I have put forward is capable of maintaining itself by its own power, but its position is greatly strengthened by direct facts. It can be demonstrated that a great inland ice *has* tried to advance from the high land westward beyond the fjord heads, and signally failed. This was the case in the last Ice Age, of which we can trace all the prominent signs. As the ice cap was not able to build itself up to any great dimensions near the western precipices to the fjords, its greatest height was piled up farther to the south-east, and the ice shed was drawn in the direction of the eastern margin, down the eastern valleys. From this time we find boulders transported up the eastern slope from a distance up to eighty miles trom the watersheds, and these boulders can be followed in great heights in the western valleys only to the fjord-heads, when they suddenly drop down to the old sea beach. This shows, beyond all doubt, that the glaciers from the second inland ice which far away to the south-east laid up the upper till in Prussia, on the north-western side, only reached the fjord-bottoms, and were not able to fill the fjords. We have in this an empirical proof of my idea. An inland ice is really not able to advance beyond a close set row of deep depressions as the Norwegian fjords.

But yet we have unquestionable proof that the exterior part of the west coast was extremely glaciated. Just in the mouth of the great Sognefjord itself we have the Sulen Isles with rock scorings and eastern erratics (but none from behind the watershed) up to 1800 feet above sea level, 5400 feet above the fjord bottom. How can these boulders have been transported across this abyss when no inland ice could ever have advanced beyond the fjord head, and no local glaciers from the peninsula on either side could ever have crossed the deep channels, or piled itself up to sufficient height here only a few miles from the steep slope to the Atlantic Ocean basin? I cannot see any way to account for these facts other than by the supposition that the fjords or depressions of the same kind and depth did *not* exist when the first great inland ice was forming, but were quite completed when the second (and last) commenced. *Ergo*, the Norwegian fjords are of early glacial or interglacial origin.

At this point of my reasoning comes in the conclusive series of arguments which puts any other origin than glacial erosion quite out of question for this peculiar flat troughs in old solid azoic and palæozoic country. I wish to lay especial stress upon the fact that Norwegian geologists for many years have laid great weight upon the really marvellous circumstance of lake-distribution only in glaciated districts, as one of the best of the many indirect proofs of the glacial origin of rock basins. I should think,



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